

# FOUNDATIONS OF PHYSICS

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INTERIM

FOUNDATIONS OF PHYSICS ( )  
FOUNDATIONS OF PHYSICS LETTERS ( )

17 January 1998

: Prof. M. L. G. Redhead  
: Dr. T. M. Ridderbos  
: History and Philosophy of Science  
: Cambridge Univ.

Dear Author:

Please carefully study the enclosed referee report(s) on your submitted paper and reply to it (them) at your earliest convenience.

Because you might receive more than one report, as well as one or more follow-up reports responding to your replies, it is important that you indicate on every reply the number of the (follow-up) report addressed by it.

It would further expedite the processing of your paper if your replies are mailed to us in DUPLICATE. Even self-addressed self-adhesive labels would be most helpful.

I thank you in advance, if I may, for your kind cooperation.

Sincerely yours,



Alwyn van der Merwe

Enc.: Report(s)

2.

2 pp. follow.

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Referee Report for  
"The Spin-Echo Experiments and the Second  
Law of Thermodynamics"

I'm not sure I really understand why the authors spend so much time developing their technical model of the time evolution of a (continuous) spin system. If one understands Liouville's theorem and the fact that because of that theorem, the fine-grained entropy of an isolated system remains constant, then one understands virtually everything the model demonstrates. It's well known that mixing leads to coarse-grained equilibrium and that the time reversed evolution leads to different results for the fine-grained and the coarse-grained probability distributions.

Also, I don't understand why the authors think it a virtue of their model that it has randomness properties no higher up the ergodic hierarchy than mixing. A result of this, as they explicitly note, is that "even for macroscopic quantities equilibrium is not being achieved at finite times." But presumably what one would like to have explained is why systems have the *finite* relaxation times that they do.

There is an important research program which tries to explain the approach to equilibrium and the finite relaxation times without appeal to external intervention. It involves appeal to stronger ergodic properties than mixing and aims to show that one can get some sort of decay of "usable" correlations from within the dynamics if the system is sufficiently randomizing. Thus K and Bernoulli systems have generating partitions which in effect enable one to individuate trajectories. Nevertheless these systems are so "randomizing" that the entire past history of the system's representative point with respect to such trajectory-individuating partitions or "coarse-grainings," is insufficient for determining which cell of the partition the point will be found in the next instant. (In the case of Bernoulli systems this insufficiency amounts to probabilistic independence.)

This research program seems like a proposal the authors should consider before concluding that an interventionist approach is the only way to explain the approach to equilibrium of thermodynamic systems.

Perhaps I'm missing something fundamental in their argument but seems to me that the authors' objections to Sklar's interpretation of the spin-echo results beg a number of questions. They argue that "first stages of the spin-echo experiments do not [contrary to Sklar] show the behaviour we normally take ourselves to be explaining. . . ." What statistical mechanics is supposed to explain is "true" equilibration and not "apparent" equilibration.

Here are several questions. First, how do we know that "true" equilibration is typical of thermodynamic systems? Second, why can't we expect statistical mechanics to explain both true and apparent equilibration? But third, and more importantly, I don't follow the authors' discussion of the "innocent" who comes upon the reversed spin system and predicts the wrong results. "The kind of thermodynamic behaviour we would like to explain using statistical mechanics is the behaviour which leads to the usual situation in which an innocent observer

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unaware of the history of the system will actually make the *right* prediction . . . It are [sic] these states which can truly be called equilibrium states." (If one defines "true" equilibrium as a state in which correlations have disappeared, it is no surprise that a situation in which Liouville's theorem applies is one where there can never be "true" equilibrium.) Furthermore, I don't see why we can't explain the innocent's surprise by appeal to the simple difference between the spin-echo systems and most others. This difference is just what the authors appeal to in saying why we are not surprised. We know that the system has been *prepared* in a special way. We can manipulate the spin systems in a way that we cannot manipulate most other thermodynamic systems. It seems to me that what needs to be explained here is the "apparent" approach to equilibrium in the first stages of the spin-echo experiments, and how the appearance of time reversed evolution is possible for the system in the short run. The explanation of how in the long run the oscillations will damp out yielding a net increase of entropy for the system and the environment (which indicates "true" equilibrium) is not of primary concern in this example.

Another question: The authors note that in the interventionist program, "the emphasis is shifted from a limited measurement resolution towards measurements which are restricted to limited, interacting systems." What justifies the limitation on the system here? Why not "expand the system" to include part of the environment? One would then need to explain the thermodynamic behavior of the "isolated" subsystem within this larger system. This is a standard objection to interventionist proposals. Why isn't the restriction to "limited" systems just as in need of justification as the coarse-graining approaches' restriction on measurement resolution? As the authors note in their later discussion of the Bergmann-Lebowitz model, the analog of the Stosszahlansatz results from the fact that "observations are restricted to the system proper" which is assumed to be in interaction with its environment. What is the independent justification for restricting observations in this manner?

Yet another question: In what sense is author's "solution" to the problem about the interventionist approach to the universe as a whole, distinct from the coarse-graining approach that they are attacking throughout the entire paper?

Finally, though I'm not convinced that there is much merit in the proposal, Prigogine et al, "A Unified Formulation of Dynamics and Thermodynamics," *Chemica Scripta*, vol. 4, 1973 discuss the spin-echo results from a rather different perspective. In effect they reinterpret entropy so as to take into account the correlations. For them, the usual macroscopic level of description is not sufficient. They offer a "solution" to the spin-echo puzzles which is not interventionist. Perhaps this proposal would be worth discussing in the current context.

*Please respond.*  
*Thank you. Cordially, A.D.M.*